2-8-1

UNITED STATES ENVIRONMENTAL PROTECTION AGENCY
REGION 10
1200 Sixth Avenue
Seattle, WA 98101

October 7, 2005

Reply to
Attn Of: ECL-111

Mike Johns
Windward Environmental
200 W. Mercer Street, Suite 401
Seattle, WA 98119

Re: Review of Food Web Model Memorandum 1: Objectives, Conceptual Model, and Selection of Food Web Model; Lower Duwamish Waterway Superfund site; Seattle, Washington

Dear Mike:

EPA and Ecology have reviewed the Lower Duwamish Waterway Group's Food Web Model Memorandum 1: Objectives, Conceptual Model, and Selection of Food Web Model, dated September 9, 2005. Our comments, which incorporate consideration of comments by the National Oceanic and Atmospheric Administration and the Duwamish River Cleanup Coalition, are enclosed. NOAA and DRCC's comments are being sent under separate cover.

We are looking forward to meeting with you on October 18, 2005 to discuss these comments. Feel free to call me at (206) 553-2140 if you have any questions about our comments.

Sincerely,

Allison Hiltner
Superfund Site Manager

cc: Rick Huey, Ecology (hard copy)
(electronic copies only):
Jennie Goldberg, City of Seattle
Jeff Stern, King County
Skip Fox, Boeing Company
Doug Hotchkiss, Port of Seattle

EPA and Ecology Comments on Food Web Model Memorandum 1: Objectives, Conceptual Model, and Selection of Food Web Model, dated September 9, 2005
October 7, 2005

Thank you for good objectives statements and Tables 5-1 and 5-2 to help us frame our set of issues for the upcoming meeting. Comments provided below are divided in to three parts:

1. A general list of questions/comments we would like to discuss at our October 18, 2005 meeting.
2. A more specific list of comments/questions based on review of the technical memorandum. These comments include items to be discussed at our October 18, 2005 meeting, and suggestions for clearer language to describe the food web model (FWM) when it is presented in the Phase 2 RI report.
3. We have also provided the list of the questions we posed to King County for the water quality data portion of our meeting

General questions/comments to be discussed on 10/18/05:

1. Concerns about sufficiency of water quality data. We appreciate that King County is coming to our meeting to discuss their past and future water quality sampling. We remain concerned that existing Duwamish water quality data may be insufficient for the FWM. If more data are needed, a sampling program to meet the needs of the FWM would have to start right away in order to provide data within the project schedule.
    a. Please describe the comprehensiveness of the dataset other than sediment and tissue data – i.e. water concentrations, water and sediment parameters (DOC, POC, TOC). There are only limited data for water and these do not cover things like seasonality, annual variability, non-resuspension flux, etc.
    b. Please explain what spatial scale will be used for water column data.
    c. We are concerned that non-resuspension flux from sediment (Thibodeaux and Bierman 2003) may be more important than fluxes from sediment resuspension. The conceptual model (Figure 3-1) should include this process. Please note that this pathway was discussed at the last LDWG FWM meeting (April 27, 2005) with Todd Bridges. Todd discussed the significant impact of non-resuspension flux on the Hudson River modeling.
    d. Please explain how water column data will be estimated in model runs for future remediation scenarios. Are you assuming that water column PCB concentrations can be estimated from sediment? If so, how will this be done? If not, what is the source for water column data in the future scenario?

2. Mechanism to assure comments are addressed. We have agreed that revisions to this and subsequent food web model memos will not occur until the final document is produded. We would like to discuss a mechanism to address comments as they are generated to improve the process as we go forward.

3. Early discussion needed. Early discussion of model parameters is needed because the agencies at this point do not have enough information to determine whether the model will

meet the objectives stated in the memorandum. It is important to start a dialogue on several issues as soon as possible, for example:

a. Please supply the list of input parameters, values, and assumptions in deliverable 2 so we can start discussions on these sooner than deliverable 3.
b. Table 5-1 - Clarify how you will calculate the site-specific input parameters (particularly for chemical concentrations in sediment, and POC, DOC in water, etc. Is the intent to use means, 95th UCLs, etc.?).
c. Over what scale(s) does LDWG intend to apply the model? What type and amount of location-specific data would be sufficient? How will that be determined/evaluated?
d. What degree of model uncertainty is acceptable in making decisions?
e. Please provide a more substantial explanation of why the underlying equations and assumptions of the Arnot and Gobas model are expected to provide a reasonable estimate across the range of expected tissue RGBs and sediment concentrations in the LDW. The memo simply states that this is the case without a supporting rationale.

4. Use of the model to evaluate remedial alternatives.
   a. Will the model be used to estimate how long it will take tissues to reach risk-based goals after sediment remediation?
   b. Will the model be able to output the results in both mass and concentration units?

5. Effect of sediment disturbance. Since there is no plan at this site to link the FWM to a sediment transport model, there should be a discussion of how sediment disturbance through prop wash, scour, and other human and natural activities would affect the conditions being modeled. (Perhaps in the uncertainty section of the FWM discussion).

6. Stakeholder concerns. DRCC's comment letter provides a good guide to issues we should be prepared to discuss at the stakeholder meeting, including:
   - A reminder of the ROCs selected for the ERA, and why they were selected.
   - Why salmon and other non-resident species are not included in the FWM.
   - Why the FWM omits birds and other higher-order receptors, and how risks to these receptors will be addressed.
   - Spatial scale to be used in the FWM.
   - How sediment disturbance affects the ability to predict tissue concentrations.
   - How juvenile salmon data will be used in the FWM.
   - Why clams are being modeled using a BSAF rather than a FWM.
   - Why bound chemicals, bacteria, and fungi are not included in the dietary uptake of demersal fish.

List of specific comments/questions based on review of the technical memorandum (including suggestions for revisions for the FWM section of the RI report)

1. **Page 1, Section 1.0, Introduction, 2nd paragraph:** Please discuss at our 10/18 meeting what the next two memoranda will cover. It appears that two of the three elements described in the first sentence (rationale for selection and modeling approach (conceptual?) are included in this first memo. What then will be presented in the second memo?

2. **Page 2, Figure 2-1:** How will performance of the model be evaluated? For example on page 9, Section 4.1: "*Model can be used to predict chemical concentrations in tissue or sediment with the degree of accuracy necessary to make RI/FS decisions.*" How will this degree of accuracy be determined?

3. **Page 3, Section 2.0, Uses and Objectives of the FWM:** To be consistent with EPA terminology, please say the FS includes an analysis of remedial (or cleanup) alternatives, not remedial designs.

4. **Page 4, Section 3.2, Species to be modeled, 1st paragraph:** The distinction being made here and in subsequent paragraphs re: target species and non-target species is confusing. The first paragraph in this section indicates that sole, sculpin, perch, and crab are either ROCs or key prey species for other ROCs. Does that mean the benthic invertebrates aren't a key prey species for ROCs? We can certainly understand how phytoplankton and zooplankton would fall under non-key prey species but we do not understand (nor agree with) that classification for benthic inverts.

5. **Page 4, Table 3-1:**
   - Clarify how porewater input concentrations will be derived.
   - Will juvenile salmon be included as a small prey species? Why or why not?

6. **Page 5, Section 3.2, Species to be modeled, 2nd paragraph:** Will phytoplankton be included as a prey item for small prey fish (as indicated in the 3rd sentence)? This is not reflected in Figure 3-2.

7. **Page 5, Section 3.2, Species to be modeled, 4th paragraph:** This paragraph is confusing since it seems to imply that clams will not be a part of the food web model. Clams have a dual role as a key prey item for humans in the HHRA and for a wildlife ROC (sandpiper) in the ERA, and as a component of the benthic market basket. Please clarify whether you are saying in the second sentence is that clam tissue concentrations ***for use in the HHRA and the ERA*** will be modeled using a BSAF. Although they will not be modeled on their own in the FWM, clams will still be considered as a component of the modeled benthic invertebrate "box" in the FWM.

8. **Page 6, Section 3.3, Table 3-2:** The information provided in this table is difficult for the uninitiated to understand. The following suggestions are designed to make the information in this table more transparent for the reviewing public. Please provide a reason why

phytoplankton and suspended detritus are lumped in the simplified conceptual model (e.g., no LDW-specific data for suspended detritus?). Clarify that the reason for lumping epibenthic and infaunal invertebrates under "benthic invertebrates" is that the samples analyzed from the LDW *combined* the two (or you could use the term "market basket benthic invertebrates"). For sediments, suggest revising the reason to be that "Analysis of LDW sediment samples did not distinguish between the sediment matrix and small organisms living in the sediment". Samples were screened to remove large organisms.

9. **Page 6, Table 3-2:**
   - Explain how you will evaluate the influence of the assumption that LDW fish consume infaunal and epibenthic invertebrates equally.
   - Clarify why the literature values for phytoplankton include all particles <236 um but do not include zooplankton in phytoplankton
   - The distinction between "water column water" and "bottom water" needs to be made clear. Note in Arnot & Gobas 2004, Table 1 - bottom water is not a parameter.

10. **Page 7, Section 3.3.2, Aqueous uptake pathways, 1st paragraph:** The last sentence should be changed to read, "The freely dissolved portion is the fraction assumed to be most bioavailable for aqueous uptake via diffusion (refs). Note that Arnot & Gobas use the term "freely dissolved" rather than "truly dissolved". Suggest keeping the terminology consistent.

11. **Page 8, Section 3.3.2, Aqueous uptake pathways, 1st incomplete paragraph at top of page:** Move the third complete sentence on this page ("Organisms that live solely in bottom water or on the sediments…") to follow the first complete sentence on this page ("Benthopelagic and demersal fish and some epibenthic…") to create a more logical progression.

12. **Page 8, Section 3.3.2, Aqueous uptake pathways, 1st complete paragraph:** The overall message and details of this paragraph are confusing. The 4th sentence needs revision to address the following questions:
   - Clarify which empirical relationships have been established and whether with porewater and/or bottom water.
   - If EqP porewater concentrations of a contaminant are to be estimated using organic carbon normalized concentrations of chemicals in sediment and Koca, how will nonresuspension flux be accounted for?
   - What's the role of the Kow and concentrations of POC and DOC mentioned in this sentence?
   - Which relationships are uncertain (as discussed in the 2nd to last sentence)?

   If the simplified food web models proposed will not include bottom water as a compartment (as is the case with the Arnot and Gobas 2003 model), then why does the last sentence of the paragraph indicate that "specified fractions" of the water column and porewater going to be used to represent bottom water in the FWM?

13. **Page 9, Section 4.2.1.1, Empirical model description:** In the future, this type of discussion should describe in more detail the approach and data manipulations, e.g., What sediment data

were used in the interpolations and how were the interpolations conducted? How was the 95% UCL computed? Were any sediment or tissue data excluded from these evaluations? Were interpolations conducted using the transformed and/or normalized concentration data? In general, we will also need the interpolation shape files and underlying attribute data.

14. **Page 10, Table 4-1**: Why was a tissue concentration of 1000 µg/kg ww chosen? The model should perform well at risk based concentrations at a risk range of 10^{-5} to 10^{-4}, as this is the likely risk range where the effectiveness of remedial actions will be evaluated.

15. **Page 12, Section 4.2.2.5, Arnot and Gobas (2004), first paragraph in section:** In the first sentence, reference is made to the SETAC Supplemental Archives S1 and S2 to support the statement that there have been improvements to data availability for model parameterization. We have printed out the entire supplemental data archive for this reference and it isn't clear why these two figures were highlighted as examples. Instead, the entire archive for the paper should be cited at the end of the sentence as follows: (Arnot and Gobas, 2004 SETAC Supplemental Data Archive, Item ETC-23-10-002; http://etc.allenpress.com).

16. **Page 13, Section 4.2.3, QEA Model:** Generally, it seems that the QEA model description is given short shrift in this memo. This isn't entirely surprising since the Gobas & Arnot model is the one being proposed for use on the LDW. Nevertheless, the section describing the QEA model and comparing it to Gobas-type models should be revised and expanded for the final deliverable to make it more explanatory (since a transparent and thorough comparison of the two models is the point of this section). For example, please clarify the comparison between the two models made in the 2nd sentence of this section ("...the QEA model simulates energy transfer while the Gobas-type models use chemical concentration..."). What is a simulation of energy transfer and what is gained by this approach? What does the Gobas-type model *simulate*? The Arnot-Gobas version of the model includes kinetic sub-models but is it still considered a steady state model? Can it, like the QEA model, be tied to a fate and transport model and run in successive time steps? What does QEA stand for and is it run as an Excel spread sheet? Basically, this section should present the information that is then summarized in section 4.3 as the basis for the decision on which model to use. It would be helpful to have a matrix comparing the various advantages and disadvantages of all models with regards to satisfying prediction needs for the LDW project. The provided text doesn't permit such a comparison.

17. **Page 14, Section 4.3, Selected Food Web Model:** Based on the information presented in the preceding section for the QEA model, it isn't at all clear why the Arnot/Gobas model is being selected. It's not that the agencies have a problem with the selection. Rather, we are concerned about the justification given here having little basis in a comparison of the strengths/weaknesses on the models. Text added to address the previous comment will help to elucidate and clarify differences between the two models that should make the selection decision more transparent and logical. In addition, the text in this section should be revised to highlight the unique qualities of the Arnot &Gobas model that make it the better tool for this project.

18. **Page 15, Section 4.3.1, Parameter uncertainty can be systematically quantified:** Please provide a reference for the San Francisco Bay application of this model (e.g., Gobas and Wilcockson, 2003).

19. **Page 16, Section 4.3.1, first paragraph**: Clarify whether the preliminary human health risk estimates involved any food web modeling.

20. **Page 17, Section 4.3.2, Application and performance of Gobas models:** It would be helpful to add text to this section discussing the issues, problems, and limitations that were encountered in the application of the Arnot/Gobas model to these sites. The sites discussed are all very large and very contaminated relative to the LDW. Could that have any effect on the relative accuracy of model predictions?

21. **Page 17, Section 4.3.2, Footnote 4:** Although we agree at this time that there is no plan to use the FWM to predict mammal, bird, or egg tissue concentrations, once we have a functional food web model, we may jointly find that it would be useful to explore effects of various remedial scenarios on mammal, bird, or egg tissue concentrations.

22. **Page 18, Section 5.0, Tables 5-1 and 5-2:** These two tables are not sufficiently detailed nor accompanied by explanatory text to be useful for the reader for comparison to the site-specific and model parameters in the Arnot and Gobas (2004) paper. In fact, when you do compare them to Table 2 of Arnot & Gobas (2004), there appear to be many model parameters which aren't included in the WindWard Memo (e.g., freely dissolved chemical concentration in pore water $C_{WD,P}$, bioavailable solute fraction φ, gill and dietary uptake rate constants k_1 and k_D, gill uptake efficiency E_w, and gill ventilation rate G_v).

    It would be helpful distinguish between those parameters that will be site-specific, those that will be derived from the literature (either as constants or species-specific values), and those that will be estimated using site-specific and/or literature values using allometric and other published relationships (e.g., k_1 , k_D , φ, E_w , and G_v). Perhaps that could be accomplished by adding an additional table?

    For each of the site-specific and literature-based parameters described in these tables, please add a column indicating the parameter name (e.g., $C_{wt,n}$ for total chemical concentration in water column; v_{LB} for lipid fraction of the organisms etc.) as specified in Tables 1 and 2 of Arnot & Gobas (2004). This will facilitate comparison between the two papers.

    Where will site- and species-specific measurements of the non-lipid organic matter fraction of an organism come from?

    Table 5-1 indicates that the concentration of particulate organic carbon in the water column will be estimated by subtracting measured concentrations of dissolved organic carbon (assuming DOC will be measured?) from total organic carbon. Do you mean TOC values from sediments or total organic carbon measurements in water samples?

23. **Page 23, Figure 3-1:** Should there be a line between POC and truly dissolved chemicals?

24. **Page 24, Figure 3-2:** Do any of the receptors consume phytoplankton?

25. **Page 24 – 26, Figures 3-2 – 3-5:** Please add incidental sediment ingestion by ROCs to these figures (and let us know how you plan to deal with this in the model).

King County Questions relative to dissolved concentration estimates of PCBs

1. KC modelers at the meeting we had a couple of years ago stated that modeled surface water concentrations (freely dissolved) in the LDW were over-predicted. How do they know this?

2. What WQ data does LDWG intend to use in the FWM? Measured or modeled data?

3. For KC WQ data that LDWG proposes to use in the FWM, please provide the following information at our 10/18 meeting:
    - number/location/dates of samples (please provide specific location information, e.g., distance from outfall, depth in water column, water depth at sampling location)
    - parameters sampled, sampling and analytical methods, and detection limits
    - dates/locations/parameters for any upcoming sampling
    - how input # and variability are derived
    - explain whether the ongoing sampling helps describe year to year and/or seasonal variability?

4. How were SPMD values translated into WQ values? Are more SPMD deployments planned or are we only talking about a single sampling event from early spring? If so, how relevant are these data for summer/low flow PCB concentrations throughout the lower Duwamish?

5. For freely dissolved concentrations, what are the estimated constraints around the values that you select?

References

Arnot JA and Gobas FAPC. 2004. A food web bioaccumulation model for organic chemicals in aquatic ecosystems. Environ Toxicol Chem. 23:2343-2355.

Thibodeaux LJ and Bierman VL 2003. The bioturbation driven chemical release process.Environmental Science and Technology. July1, 253A-258A.